modulates rat vascular smooth muscle cell intracellular calcium metabolism. J Nutr 1998;128:180–4.

Chromium picolinate and type 2 diabetes

Dear Sir:

The article by Althuis et al (1) in a recent issue of the Journal appears to be an imbalanced review of the publications to date regarding the potential benefit of chromium picolinate in persons with type 2 diabetes. Among the current published works, there are at least 9 noteworthy reports of clinical trials that show the relative efficacy of chromium picolinate (2–10). These studies concentrated on the effects of chromium picolinate on markers of blood glucose or on insulin regulation in subjects with type 2 diabetes or in persons with induced diabetes. The major fault in the conclusions of Althuis et al is that no studies of persons with diabetes were included in their final analysis.

In reading the 9 reports, it is easy to see that 1349 total subjects were studied over the past 10 y. With such a large number of subjects having participated in single- and double-blind trials, the findings are consistent: chromium picolinate has a positive effect on fasting insulin values and on hemoglobin A_1C. The data also indicate that, when used with standard treatments, chromium picolinate improves clinical results (eg, those for biguanides, sulfonylureas, or metformin alone) (10). Additional benefits have been found with chromium picolinate supplementation for coronary disease risk profiles [ie, lipids and lipoprotein(a)] that are important in the diabetic and nondiabetic communities. It is agreed that the dose for clinical benefit has not been universal, ranging from 200 to 1000 μg, but this only shows that "one size does not fit all," and thus a dose that is dependent on body surface area is indicated.

In any event, with the relative safety and inexpensiveness of chromium picolinate, there seems to be no reason for it not to be used in people who have poor blood sugar control or insulin resistance syndrome (11, 12). The benefit-to-risk ratio favors benefit. Continued research on the positive effects of chromium picolinate on biomarkers of blood sugar regulation is needed to expand the body of evidence for its utility as an adjunctive treatment of conditions that affect blood glucose. In addition, because the current data imply that some people respond better to chromium picolinate than do others (nonresponders), it may be that a test to identify the best candidates for treatment with chromium picolinate is indicated. However, we as scientists and clinicians cannot dismiss the current body of work that indicates the efficacy of this mineral, nor should we dismiss consumer support for this product as being without merit.

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Reply to DS Kalman, MF McCarty, and V Juturu and JR Komorowski

Dear Sir:

We thank the authors of the letters for their comments on our article. Our review and meta-analysis summarized randomized clinical trials (RCTs) designed to assess glucose and insulin responses to dietary chromium supplements (1). We limited our review to RCTs to avoid the potential for bias inherent in nonrandomized studies. We attempted to include every RCT in the literature.

The letters by Kalman and by Juturu and Komorowski cite several studies they say our review omitted (2–11). Our review in fact included 4 of these studies (2–5); the other 6 studies they mentioned (6–11) were not RCTs and therefore were not eligible for inclusion. Specifically, 1 of the 4 RCTs cited as being omitted was both discussed in the review and combined analytically in the meta-analysis (3). In addition, we discussed in detail the findings from the other 3 RCTs although they were not included in the meta-analysis (2, 4, 5). One of these RCTs (4) was excluded from the meta-analysis because the study population—women with gestational diabetes—was not a focus of our review; one of the others was excluded simply because data presented in the original report were insufficient for abstraction, and updated data were not available from the investigators (5).

The authors of all 3 letters express concern that we did not analytically combine the study by Anderson et al (2) in the meta-analysis. First, the Chinese population described by Anderson et al was very different from the populations of the other trials, such that its inclusion would lead to violation of the statistical assumption of heterogeneity in models pooling all 4 studies. Second, because the odds ratio estimated by meta-analytic techniques is weighted more heavily for large studies, pooling the data from Anderson et al ($n = 155$) with the data for the 38 subjects from the other 3 studies would overwhelm the results, making the effects of the smaller studies—ie, studies from populations more similar to that of the United States—difficult if not impossible to assess. Thus, we believe that separating the presentation of the results of the Western studies from that of the results from the one non-Western study better facilitates critical review.

The remaining 6 studies cited as being omitted were not RCTs, but rather uncontrolled investigations (6–11). In addition to an uncontrolled study, one report described a small controlled clinical trial that assessed 10 subjects who were not randomly assigned to receive treatment or placebo (11). Although the data were not presented in the report, those authors reported no difference between the placebo and chromium groups (11).

McCarty is correct that the studies we reviewed did not address the use of high doses of chromium. He points out that data from uncontrolled and animal studies suggest that chromium may be valuable as a dietary supplement or in pharmaceutical doses. Nonetheless, before making recommendations for use by the general public, we urge that investigators test dietary chromium supplements, particularly those with high doses, in a well-designed RCT. The limited data from RCTs on dietary chromium supplementation have yet to prove that it is either efficacious or safe for healthy persons or for those with type 2 diabetes.

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Relation between physical activity and obesity

Dear Sir:

It was with great interest that we read the recent publication by Ekelund et al (1) on reduced physical activity in obese 18-y-old adolescents. In this carefully designed study, the authors measured physical activity by using the doubly labeled water method in conjunction with accelerometry. They discerned that obese adolescents are less physically active than are matched control subjects, despite no significant differences in the energy cost of physical activity between the groups. In addition, the physical activity level (PAL) was lower in the obese group than in the control group, and there was a negative relation between PAL and percentage body fat in the cohort. As the authors point out, many contradictory results in the literature show positive, negative, or no relations between physical activity and adiposity, including our own results, which showed no relation between activity and adiposity in 5-y-old Pima Indians (2). Ekelund et al also note that in cross-sectional studies such as their own, it is not possible to discern whether an inactive lifestyle causes obesity or whether obesity causes an inactive lifestyle.

We recently published a follow-up study of the same Pima Indian cohort, in whom metabolic measurements were made at 5 and 10 y of age, which may cast some light on this relation (3). At 5 y of age, there was little relation between PAL and adiposity (percentage body fat by dual-energy X-ray absorptiometry: $r = -0.05$, $P = 0.55$); however, at 10 y of age this relation was significant and negative ($r = -0.28$, $P = 0.05$). In keeping with this, 5-y-old children who were “overweight” [by National Center for Health Statistics criteria: body mass index (BMI) ≥ 95th percentile] or “at risk of overweight” (95th > BMI ≥ 85th percentile) showed a very modest rise in PAL over time, whereas in children at low risk (BMI < 85th percentile), PAL increased by 70% over baseline by age 10 y. PAL at age 5 y, which tracked only modestly to age 10 y ($r = 0.34$, $P = 0.008$), was not an independent predictor of weight gain by 10 y of age.

Our study illustrates many points. First, our results show that PAL, at least at age 5 y, is not an important correlate of adiposity